THE EFFECT OF IMPERCEPTIBLE SHADOWS ON THE JUDGMENT OF DISTANCE.

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Some six years ago, an investigation was published under the above title from the Psychological Laboratory of the University of California.[1] Briefly stated, its thesis was that a motive to optical illusion, although so faint as to be wholly imperceptible to the observer, is nevertheless effective in the determination of judgments of visual distance. One is required to compare, under certain methodical conditions, the lengths of two contiguous sections of a straight line. To all appearances, the stimuli are perfectly simple: one sees, drawn horizontally upon a white background, a thick black line which is bounded and divided by three vertical black marks; and the problem is, in successive observations, to report upon the equality or difference of the two sections thus displayed. The peculiarity of the experiment lies in the fact that the stimuli are only apparently simple. In certain series, not known to the observer, the experimenter throws upon the white background angular shadows, disposed in such a way as to convert the two lengths of line into the two parts of the Müller-Lyer illusion. These shadows, be it repeated, are so faint that they are never, even under the greatest strain of attention, visible to the observer. Yet they have their due effect: the judgments of length of line prove to be subject to a constant error, whose sign, *plus* or *minus,* reflects the tendency of the motive to illusion.

This contention is, in itself, startling enough. A great deal has been written, of recent years, about the subliminal and the subconscious, and many wonderful things have been declared in their name. Where the phenomena are obscure, the definitions arbitrary,

[1] K. Dunlap, in *Psychological Review*, VII., 1900, 435 ff.

and the training and temperament of the writers markedly diverse, much variety of opinion will ensue, and dogmatism is altogether out of place. But the case now under consideration is unique.[1] It is asserted that in straightforward psychophysical method-work, done under strict conditions, the eye may be solicited by lines which it cannot see, the judgment warped by a motive which is neither in consciousness at the time nor ingrained by habit in the nervous system of the observer.

A principle so revolutionary—for the whole environment is full of subliminal influences, which experimental psychologists have systematically neglected!—must, one would think, be based upon unequivocal evidence. Hardly less surprising, now, than the conclusion itself are the numerical results which claim its acceptance. The standard length of line, throughout the experiments, was 25 cm. The errors of judgment ascribed to the illusion motive vary between the extreme limits of 1.05 mm. and 0.10 mm.[2] "The difference," as the author admits, "is slight; but," he adds, "we should hardly expect to get more than a slight effect from the shadows under the circumstances."[3] The effectiveness of an optical illusion, that is to say, stands in direct proportion to the clearness of contour of the figure shown. Is it not more reasonable to suppose that an illusion motive, if effective at all, will be effective at full strength? Or, at any rate, is not this alternative supposition worthy of experimental test?

It is, however, easier to accept a statement once made—especially if the content of the statement fall in with our immediate intellectual purpose—than critically to estimate the value of the evidence upon which the statement rests. And Dunlap's thesis has, accord-

[1] Dunlap finds a parallel to his own results in the experiments of C. S. Pierce and J. Jastrow on small differences of sensation (*Memoirs of the National Academy of Sciences*, III., i., 1884, 75 ff.). There is however, no resemblance whatever between the two investigations. As the latter has been discussed by G. E. Müller in 1904 ("Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik") and by Titchener in 1905 ("Experimental Psychology," II.), and as it contains nothing which can invalidate the canons of psychophysical method, we leave it here out of account.

[2] Dunlap, *op. cit.*, 448. The figures given are: 0.62, 0.40, 0.17, 0.83, 0.18, 0.10, 1.05 and 0.72 mm.: an average of half a millimetre!

[3] *Op. cit.*, 450.

ingly, found its way, as matter of proved fact, into the current 'literature' of psychology.[1] This circumstance alone is a justification for the reopening of the whole question.

Apparatus and Method.

It is clear that the conditions of the experiment may be met in two different ways. Either the white background may be made translucent, and the shadows shown from behind by transmitted light; or the background may be made sensibly opaque, and the shadows thrown upon it from the front. Both forms of apparatus were employed by Dunlap: our own experiments, for reasons which will be given later, were performed only with an apparatus of the first type. In order that the two sets of results might be comparable, we at first set up this apparatus in accordance with Dunlap's description, and reproduced his method of work.

A sheet of white bristol-board, 58 by 72 cm., was mounted horizontally in a black wooden frame. Across the middle of the sheet was drawn a black line, 1 mm. in width, which was crossed at the centre by a vertical black line, 0.75 mm. wide and 17 mm. long. To the sides of the frame, at the level of the horizontal line, were fastened guides, in which ran small rods of blackened steel: the rods could be drawn in or out along the line, and were of course invisible against it. The inner ends of the rods carried vertical strips of black paper, 2 mm. wide and 10 mm. long. These strips, like that at the centre, extended equally above and below the horizontal line; and the three together marked off the two sections of that line whose lengths the observer should presently be called upon to compare. A mm. scale, attached to the frame above the rods, and a small marker on the rods themselves, made it possible to set the limiting strips, with accuracy, to any required position.

[1] We may mention G. M. Stratton, "Experimental Psychology and its Bearing upon Culture," 1903, 88 ff.; J. Jastrow, "The Subconscious," 1906, 417 ff.; J. B. Pratt, "The Psychology of Religious Belief," 1907, 20. Dunlap's results are accepted by M. Meyer ("Aus den Versuchsergebnissen geht mit Deutlichkeit hervor, dass auch in solchem Falle die Illusion stattfindet"), in *Zeits. f. Psychol. und Physiol. d. Sinnesorg.*, XXV., 1901, 266; by A. Meyer, in *Journal of Philos., Psychol. and Sci. Methods*, IV., 1907, 80 f.; by the anonymous reviewer in *Mind*, N. S., X., 1901, 281; and, although with more reserve, by J. Philippe, in *Rev. philos.*, 1901, 341. We are not aware that they have as yet been incorporated in any formal text-book of psychology.

The shadows were cast by angular pieces of stiff black cardboard (angular magnitude, 60°; length and width of legs, 45 and 5 mm. respectively). One of these was fastened to the back of the sheet, its vertex coinciding with the centre of the central vertical line on the front. The other two moved in or out with movement of the limiting strips. The arrangement was simple: the rods carrying the strips were bent round, to the back of the frame, and there passed through guides placed at the level of the front guides and strictly parallel to them. The cardboard angles were attached to the inner ends of this second pair of rods, with their vertices at the centres of the strips. Whenever, then, the limiting strips were moved, their angles moved with them, and strip in front and angle behind maintained always the same relative position.

The apparatus thus constructed was set up on a table in a dark room. Its front surface was illuminated by two hooded incandescent lamps, placed symmetrically on either side and at equal distances from the sheet; its back surface was illuminated by a single hooded lamp, placed opposite the centre of the sheet. This third lamp was controlled by a rheostat, and all three were on the same electric circuit. The bulbs were of ground glass, and the light of the lamps was further diffused by sheets of tissue paper.

The procedure was now as follows. The observer, seated before the apparatus, was left in the dark for 15 minutes, in order that his eyes might be properly adapted for the experiments. The lights were then turned on, and the experimenter held up, directly behind the frame, a circle or a skeleton square cut from black cardboard. The intensity of the light at the back was slowly reduced, until the observer was just unable to detect the shadows cast by these figures. Or, rather, the reduction of the light was arrested at a point somewhat short of this: at the point, namely, when the observer declared that there might perhaps be a shadow there, on the white background, but that he could not possibly decide whether it was the shadow of a square or of a circle. We may say at once that the arrest of the rheostat at this point caused us some inconvenience in the experiments proper, since it not infrequently happened that the observer remarked, in the course of a series, that he thought he saw a shadow on the white sheet: in which case the

series was thrown out, and repeated later on. We wished, however, to give the subliminal shadows every chance to produce their effect, and were therefore content to run this risk of additional work.

Having regulated the intensity of the light behind the apparatus, the experimenter affixed the Müller-Lyer angles to the sheet and rods. The constant or standard line, which was placed as often to the right as to the left, was 25 cm. in length. The variable line was set, at the beginning of a series, to appear either as distinctly longer or as distinctly shorter than the standard; and the limiting strip was moved in or out, by steps of 1 mm. and at intervals of 15 seconds, until several judgments had been recorded of the opposite tenor to that with which the series began. Every series taken with the angles was paired with a precisely similar series taken without them.

The order of experimentation was, so far as possible, left to chance. Thus, the position of the angles for a given paired series, as open or closed, was determined by lot. There were as many series beginning with "longer" as with "shorter," but their distribution was also determined by lot. Finally, while the members of a paired series were always given together, chance was allowed to decide whether the shadow-series should precede the shadowless, or conversely.

The judgments of the observer referred always to the variable line, and took the form "longer," "shorter," and "equal"; or "doubtful." The middle point of the region of doubt and equality was taken as the mean equality point of the single series, and this was compared with the mean equality point of the other member of the pair[1]. Five observers took part in the experiments: Mr. L. R. Geissler, Mr. C. R. Hugins, Professor T. A. Hunter, Mr. W. H. Pyle and Miss E. A. Smith. All except Mr. Hugins had had extended training in psychological observation.

[1] In the foregoing, we have closely followed Dunlap: *op. cit.*, 436 ff. The principal differences of procedure appear to be these: that we gave our observers a fairly extensive preliminary practice; that we allowed a period for adaptation of the eyes to the dark; and that we placed the standard line as often on the one side of the figure as on the other (for Dunlap, the standard was always on the left).

Experiment I.—What, now, of the results? Dunlap quotes no figures. He merely says that "a set was counted 'for' or 'against' the illusion according as the difference between the mean equality points was or was not in the direction which would correspond to the possible effect of the illusion-figure."[1] And he tabulates his results as follows:

Observer.	Paired Series.	For Illusion.	Against.	Neutral.
A	23	14	8	1
R	11	9	2	0
S	13	8	5	0

Nothing is here said of the magnitude of the illusion-effect. If, however, this point is disregarded, and our own results are treated as Dunlap prescribes, we obtain the following table:

Observer.	Paired Series.	For Illusion.	Against.	Neutral.
G	8	4	3	1
Hg	12	5	5	2
Hn	12	3	5	4
P	10	4	4	2
S	12	4	7	1

It is plain that the two sets of results are not in agreement. Since apparatus and method were practically the same, and care and skill in the conduct of the experiments may be assumed to be equal, we can only conjecture that the positive testimony of Dunlap's series is due to chance, operating on a small group of observations.

Experiment II.—It would, naturally, have been simply a matter of time to extend these series to a point at which the effects of chance should be ruled out. It seemed advisable, however, to modify the procedure. Dunlap himself refers to the experiments above reported as "preliminary to the investigation proper," and asserts that the apparatus was "not very satisfactory in its operation":[2] though he gives no details. We found two principal sources of error. On the one hand, we feared that the presence or absence of light behind the screen, as the change was made from shadow-series to shadowless, might influence the observer's judgment. We

[1] *Op. cit.*, 439.
[2] *Op. cit.*, 439, 436.

decided, therefore, to keep the illumination constant, and to remove and attach the Müller-Lyer angles as occasion demanded.[1] On the other hand, there can be no doubt that the direct pairing of two precisely similar series is methodically indefensible; the observer tends to say 'equal' or 'doubtful,' in the second series, at about the same point at which he passed this judgment in the first.

The method that would naturally be employed in an investigation of this sort is the method of constant-*R* as applied to the determination of equivalent stimuli.[2] We desired, however, to keep as closely as possible to the method chosen by Dunlap, and accordingly proceeded as follows. For each observer we made out a set of twenty-four single series. In eight series, the angles were used with the "illusion long" as the variable. In eight series, the angles were used with the "illusion short" as the variable.[3] In the remaining eight series, the illusion-angles were not used. On the basis of the previous experiments we selected eight different starting-points for these series, four lying well without and four well within the point of subjective equality. The order of the single series was decided by chance; the variable was shown as often on the right as on the left. The intervals between series were kept constant, so that the observer had no means of knowing whether or not the experimenter changed the apparatus. For the rest, the observations were taken and the calculations made as in the preliminary experiments.

The table shows the results obtained from five observers: the *G*, *Hg*, *Hn* and *P* of the former experiments, and Mr. R. W. Sailor, a trained observer.

[1] It must be remembered that, though the observers were not informed of the object of these experiments, and (with the exception of *P*) were unfamiliar with Dunlap's work, they nevertheless received a fairly definite suggestion from the preliminary tests with the circle and skeleton square. No one of them reported any difference in the appearance of the white background from series to series. Two, however, differentiated the series by the glow of light shed upon the table by the back lamp when this was turned on. We arranged black curtains to cut off this diffused light; but there was still a faint glow upon the walls of the room.

[2] Titchener, "Experimental Psychology," II., 1905, i., 104; ii., 258.

[3] Dunlap, *op. cit.*, 437. In Dunlap's use of the phrases, the reference is always to the left or standard segment of the line.

Observer.	Average Setting of Variable with		
	Illusion Long.	Illusion Short.	No Illusion.
G	249.8 ± 2.5 mm.	251.4 ± 2.8 mm.	250.1 ± 2.5 mm.
Hg	252.8 ± 1.8	249.5 ± 1.8	249.8 ± 1.4
Hn	251.7 ± 2.0	246.6 ± 0.9	249.2 ± 2.5
P	250.0 ± 0.9	250.0 ± 1.1	249.6 ± 2.1
S	250.0 ± 1.5	248.0 ± 2.2	250 5 ± 2.0

If the illusion motive is effective, the figures of the first column will be less, those of the second greater, than the corresponding figures of the third column. The observers *Hg* and *Hn* thus give negative results; *P* and *S* give one positive and one negative; and *G* alone gives two positive results. But a glance at the magnitude of the differences, as compared with that of the *MV*, shows the absurdity of drawing any conclusion of this sort from the results.[1]

Experiment III.—In view of the negative outcome of this second set of experiments, it seemed necessary to raise the question suggested in the introduction to this paper: the question whether an illusion motive, if effective at all, must not be effective at full strength. Have we any reason to suppose that a dim illusion-figure will produce a small effect, and a clear illusion-figure a marked effect upon the observer's judgment? We approached this problem as follows.

The horizontal line and its three markers were removed from the white bristol-board, and their place was taken by a length of fine black sewing thread. To render the shadows of the Müller-Lyer angles intensive, we placed three lamps behind the apparatus,

[1] The averages, as was stated above, have been calculated in Dunlap's way. We have submitted the figures to all the other forms of methodically permissible treatment known to us, and can discover no trace of the influence of the illusion motive.

Jastrow declares (*op. cit.*, 417) that if, after experiments with the Müller-Lyer figure, one proceeds to experiment upon the estimation of visual distances in Dunlap's way, *i. e.*, with "the shadow-strokes reduced to such a degree of faintness that the eye fails to detect their presence," the observer will judge "naturally with diminished confidence" as to the relation of the two lines. Why? If the shadows are not seen, how can the observer's confidence be diminished? He is simply called upon to compare two horizontal lines.—On the general question of 'degree of confidence,' see esp. G. E. Müller, "Gesichtspunkte u. Tatsachen der psychophysischen Methodik," 1904, 21 f.

and darkened the two lamps in front by curtains of black cloth. Under these conditions, the shadows stood out sharp and crisp upon the thread line. To render the shadows faint, we reduced the intensity of the single light at the back, as in the previous experiments, until the angles were barely perceptible. These faint shadows were, of course, stronger than the shadows of the earlier experiments. In the latter, the light was so far reduced that, at best, only shapeless patches of bright grey could be discerned upon the white background. In the present series, the shadows were still seen as angular strips of very light grey. At the same time, they were so faint that they frequently faded out, in whole or part, during the progress of a series. If, then, the efficacy of the illusion motive varies with intensity of stimulus, there should be a wide difference in the results of experiments carried out at these two extremes of the intensive scale.

As the illusory effect of the Müller-Lyer figure may decrease with practice,[1] we thought it well to secure the services of naive and untrained observers, in order that we might compare their judgments with the judgments of some of the trained observers already at our disposal. Experiments were made with two unpractised observers, Miss G. M. Fairlamb and Mr. G. W. Hau. Of *H*'s results we shall speak presently. The average effect of the illusion motive in the first eight series taken with *F* was

Shadows Weak.	Shadows Strong.
30.6 mm.	41 mm.

There was thus a distinct difference in favor of the stronger shodows. Nevertheless, the high value of the *MV* in the series with weak shadows, the length of time required for the passing of judgment in the critical zone, and the observer's complaints of the fluctuating character of the shadows, showed that the two series were not

[1] C. H. Judd, *Philosophical Review,* ix., 1902, 27 ff. Judd's law of decrease with practice is not universal, as is proved by the fact that the magnitude of the Müller-Lyer illusion-effect in the case of one of the writers (T) has shown a slight but constant *increase* with increase of practice. Cf. V. Benussi, "Zur Psychologie des Gestalterfassens," in A. Meinong's "Untersuchungen zur Gegenstandstheorie und Psychologie," 1904, 331 f. In general, however, we agree with Judd and F. Schumann (*Zeits. f. Psych. u. Physiol. d. Sinnesorg.,* XXX., 1902, 263 f.) that, with spontaneous reaction to the figure, practice tends to reduce the illusion-effect.

strictly comparable; we had made the weak shadows too weak for our direct purpose. At all events, the illusion-effect of 30.6 mm. with the weak shadows is a large effect, and it is hardly possible that the further weakening of the shadows, to the point realised in the previous experiments, should, if the illusion motive is effective at all, reduce this effect to a magnitude smaller than the *MV* of practised observers. To make assurance doubly sure, we took twenty-four series of experiments with *F* under the original conditions, and obtained the results:

Average Setting of Variable with		
Illusion Long.	Illusion Short.	No Illusion.
249 ± 2 mm.	249.7 ± 1.2 mm.	250.5 ± 1 mm.

That is to say, there is no evidence of any effect at all exerted by the imperceptible shadows.

F's practice was continued, until the magnitude of the illusion-effect was approximately the same for her as for our more practised observers. We finally obtained the following average values for the illusion:

Observer	Shadows Weak.	Shadows Strong.
F	9.1 mm.	12.7 mm.
G	12.1	10.3
P	11.1	16.9

It is an irony of chance that the observer *G*, whose results have so far been (in Dunlap's sense) accordant with the hypothesis of the influence of imperceptible shadows, should here give a larger illusion-effect with the faint than with the strong shadows.

The table is not altogether satisfactory, because the weak shadows were, throughout, fluctuating; we had made them a little too weak for comparative purposes. But the main point is clear: even on the very edge of imperceptibility, the weak shadows have an effect that is of the same numerical order as the effect of the strong shadows, and this with observers whose judgments show no influence of imperceptible shadows.[1]

[1] The figures of the table represent the average value of the illusion-effect as drawn from 16 methodically planned series, in 8 of which the variable gave the "illusion long," and in 8 the "illusion short." The smaller values obtained for the illusion-effect with shadows weak, in the cases of *F* and *P*, are accounted for by the fact (attested both by measurement and by introspection) that in a few series the tips of the shadows were so faint that the illusion motive was disregarded by the observer.

Experiment IV.—*H*'s results were, from the first, radically different from those of *F*. Whereas *F* showed an initial illusion-effect of about 35 mm., *H*'s first two series, with the shadows strong, gave a variable line of

Illusion long.....248 mm.
Illusion short.....255 mm.

and later series yielded results of the same order. That is to say, the shadows, in both sets of experiments, were practically ignored. *H* explained in the vernacular that "we couldn't fool him with those shadows"; and the event proves him right.

These particular shadows, it will be remembered, lay upon the line of sewing thread, which was itself relatively narrow, and which had no vertical markers. If the shadows might be ignored, or abstracted from, under conditions thus favorable to their influence, it seemed to us that they might still more easily be ignored under the conditions of the earlier experiments, in which the horizontal line was relatively wide, and the three vertical markers stood out clearly upon the white background as the limits of the compared distances. To test this theory, we restored the apparatus to its original form, and made a series of experiments with one of our practised observers, Mr. Sailor. The instructions were that no attention should be paid to the shadows, but that judgment should be passed upon the lengths of the lines simply by reference to the position of the vertical markers. The results were as follows:

	Setting of Variable Line with Shadows Weak.	Shadows Strong.
Illusion long.....	250.5 mm.	251 mm.
" "	248	247
Illusion short.....	251	248
" "	252	249

The moral is clear. The observer is here able, by direction of attention, to resist the solicitation of a strong illusion motive, clearly presented. So much the more then will he, under the conditions of our first experiments, resist the solicitation of an illusion motive which he cannot see, of whose presence in the particular series he is entirely ignorant, and which is left out of account in the instructions given him by the experimenter.[1]

[1] *H*'s tendency spontaneously to ignore the illusion-motive from the outset is an interesting fact. One of the writers (*T*) has come across other

Summary and Criticism.

We have now shown:

1. That imperceptible shadows, raised almost to the limit of perceptibility, exert no influence upon the judgments of distance passed by five observers;

2. That shadows, so weak as barely to hold their form distinct, exert an influence upon judgment comparable with the influence exerted by strong and clear shadows;

3. That it is possible, by voluntary direction of attention, to free the judgment from the influence of a clear and strong illusion-motive.

In other words, we can find no experimental confirmation of Dunlap's results, and we believe that a more exact analysis of the conditions of his experiment shows these results to be illusory. We suggest, further, that imperceptible shadows, if they affect judg-

instances of it; but it does not appear to be common. Judd remarks (*op. cit.*, 38) that "early in the practice series both observers noted the feeling of having succeeded in abstracting from the oblique lines. That they had not done so appears in the fact that the illusion continued in almost its full original strength." There are, evidently, individual exceptions to the general mode of apprehension of the regular Müller-Lyer figure by unpractised or little practised observers.

We could not, then, generalise from *S*'s results, if the figure employed had been the regular Müller-Lyer figure. But, as is stated in the text, the figure employed was in so far different that the three vertical markers on the front of the screen afforded definite resting-places for the eye. The shadows were not, so to say, integral parts of the total figure shown; that figure was, first of all, a black line, with a long thin vertical at its centre, and short thick verticals at its two ends: the shadows were secondary. Under these conditions, abstraction from the shadows, with definite instructions from the experimenter to that effect, offers no special difficulty: *S*'s results were, as a matter of fact, confirmed by unsystematic experiments made with two other practised observers.

H evidently represents a case of self-suggested *A-Reaktion* (in Benussi's terminology), that is, of the reaction in which "die Versuchsperson . . . die Hauptlinie der Figur als einen selbständig und isoliert vorliegenden Gegenstand erfassen muss" (*op. cit.*, 310). He would not be 'fooled' by the shadows; he directed his attention to the horizontal line. His and *S*'s results agree with those of Benussi's prescribed *A*-reactions: "in der Tat hat Judd ungefähr 1500 Einstellungen gebraucht, um die Täuschung auf einen Wert zu bringen, der sich bei vorgeschriebener A-Reaktion nach *einigen* Einstellungen erreichen lässt" (*op. cit.*, 332).

ment at all, must affect it by more than the 0.5 mm. in 25 cm. which is the average of Dunlap's observations.

It remains, now, to seek an explanation of Dunlap's positive results. We said above that the method pursued in these experiments is not the method best suited to the problem. Similar exception may be taken to the apparatus. For while the setting of the distances is sufficiently accurate, the illumination is not under measurable control. We have spoken of "barely perceptible" shadows, but we have not been able to specify the amount of light thrown upon the back of the apparatus in any given series. We do not think that this lack of quantitative control at all invalidates our results; but we confess that, from the physical point of view, the experiment would have been prettier had such control been exercised.

Dunlap's work, after his preliminary experiments, was done with an apparatus in which the shadows were thrown upon the white screen from in front, and the amount of light employed to produce them was measured by means of an episcotister.[1] We did not reproduce this apparatus, partly because our results seemed conclusive, partly also because the apparatus is cumbrous, and appears likely to introduce new sources of error. We have still, however, to account for the positive outcome of Dunlap's investigation.

We grant, at once, that we can give no single or convincing explanation of these figures. All that we can do is to suggest the various possibilities of explanation that have occurred to us. Thus, (1) the average illusion-effect is, as we have pointed out, 0.5 mm. upon a standard line of 25 cm. Dunlap nowhere gives his *MV*; but there are indications in the paper that it must have been, at the least, as large as our own.[2] An illusion-effect of such inconsiderable amount, absolute and relative, might very well be ascribed to chance. (2) It is conceivable that the figures rest upon a miscalculation; experimental psychologists, from Fechner down, have been liable to slips in addition and subtraction. Nor is this suggestion as gratuitous as it may at first sight appear; for the paper

[1] *Op. cit.*, p. 440.
[2] *Op. cit.*, 445 f., 447 f.

shows at least two instances of careless handling. If the two plates on pp. 449, 451 are compared with the description on pp. 452 f., it will be seen that the lower diagrams of each plate have been interchanged: Figs. 7 and 8 should be Figs. 11 and 12, and conversely.[1] And again: if the table on p. 448 is scrutinised, two mistakes will be noticed. The difference between + 2.65 and + 1.75 is given as — .10; the difference between + 1.06 and + 0.78 is given as — .72. It is easy to read 2.65, 2.75, and 1.06, 1.78: but then the differences, instead of being *minus,* are *plus,*—that is to say, tell against Dunlap's conclusion. At all events, something is wrong, either with the principal figures or with their differences. (3) Dunlap's observers showed a progressive change of judgment throughout the experiment. Whatever may be the explanation of this change,[2] he tells us that three of his observers overestimated the right segment early in the experiment, and later underestimated it; while the fourth observer, "with a single exception, overestimated the right segment throughout the experiment, rather more

[1] Dunlap says that 5, 8, 9 are indifferent; 6, 10, 11 faintly in accord, and 7, 12 in striking agreement with his hypothesis. If we read 5, 12, 9; 6, 10, 7; and 11, 8, we bring the plates into accordance with the text. These changes, however, mean the replacing of the present 7, 8 by 11, 12, and conversely.

[2] The explanation must probably be sought in a general tendency of judgment, complicated by preferential direction of attention: it will be remembered that the experiments were doubly one-sided, in that (1) the standard line was always shown on the left, and (2) the variable was always increased from "shorter" to "equal," never reduced from "longer." The shift of judgment might have been checked by suitable instruction from the experimenter (Titchener, "Experimental Psychology," II., 1905, ii., 305 f.). How far practice was involved it seems impossible to say.—It is, of course, theoretically possible that the minute values obtained by Dunlap for the illusion-effect are due to a very high degree of practice with the Müller-Lyer figure. We have not seriously considered this possibility, (1) because Dunlap says nothing of preliminary practice; (2) because he gives no intimation that the illusion-values of his Table I. were different in kind from those of Table IV (see p. 450): (3) because he definitely ascribes the small values to the "circumstances" of the experiment, i. e., to the subliminal character of the shadows (p. 450): and (4) because, in view of Judd's and of our own results (our observer *G*, in particular, has had extended practice with this illusion-figure), we do not consider that Dunlap's experiments were numerous enough to reduce the illusion-average to 0.5 mm.

toward the last than the first."[1] For this reason, "in computing the averages, those for the shadowless series which were taken at the same time as the series with the illusion 'long' were kept separate from the averages for the shadowless series taken at the same time as the series in which the illusion was 'short.'"[2] It is clear, however, that shadowless and shadow-series could not, in strictness, be taken at the same time; they were taken successively. And, as the change of judgment was progressive, Dunlap's averages are used for the comparison of results that are, in strictness, incomparable. Under these circumstances, it is entirely possible that chance, in determining the order of the single series, may have played, so to say, into the hands of the illusion motive. (4) Dunlap does not tell us how he measured his lines: whether behind the screen, from angle to angle, or in front of the screen, from marker to marker. If he measured behind the screen, then the movement of the right-hand angle only every fifth time that the right-hand marker was moved[3] would introduce a constant error, which must, necessarily, operate in the same direction as an effective illusion motive. (5) Lastly, it may be observed, in general, that observers in method-work, however well-meaning, fall easily into a reliance upon secondary criteria; and that an apparatus of the kind used by Dunlap might easily admit this source of error. This suggestion must remain vague, since, without actual trial of the apparatus, we cannot say what the nature of the secondary criteria would be; the suggestion itself, however, does not seem to us unfair, whether in the light of our own experience or in that of Dunlap's account of his procedure.

To attempt, in this manner, to explain away the results obtained and the conclusions offered by another investigator is not a grateful task. Some of our suggestions may be put out of court at once by a word of explanation from Dunlap. The suggestion of a possible miscalculation —made by us, be it repeated, only on the ground of positive evidence of careless treatment—should be offset by the admission that Dunlap planned his experiments carefully, and with

[1] *Op. cit.*, 447 f.
[2] *Op. cit.*, 448.
[3] *Op. cit.*, 442.

due regard to the dangers of bias and partiality. But in any event, whether or not we have hit upon the right explanation of his results, there can be no doubt that these results are themselves untenable. Our own experiments point unequivocally to the one conclusion that, if the subconscious is to be received into experimental psychology at all, it must find some other means of access than these imperceptible shadows.